MICHIGAN ENVIRONMENTAL SCIENCE BOARD

AIR PANEL MEETING SUMMARY
MONDAY, JUNE 23, 1997
INSTITUTE FOR ENVIRONMENTAL TOXICOLOGY
MICHIGAN STATE UNIVERSITY
EAST LANSING, MICHIGAN

PANEL MEMBERS PRESENT
Dr. Lawrence Fischer, Chair
Dr. Raymond Demers
Dr. Jack Harkema
Dr. Ralph Kummler
Dr. Ken Rosenman
Dr. George Wolff
Mr. Keith Harrison, Executive Director

DEQ/OSEP SUPPORT STAFF PRESENT
Mr. Jesse Harrold, Environmental Officer
Ms. Patricia Hiner, Executive Secretary

I. CALL TO ORDER

Dr. Lawrence Fischer called the meeting to order at 10:00 am.

II. PANEL DISCUSSION

Dr. Fischer asked whether Mr. Harrison had received any inquiries from the Governor's office regarding the status of the report. Mr. Harrison indicated that the Governor's office would definitely like to get the report before the United States Environmental Protection Agency (USEPA) announces its decision on July 19, 1997. Dr. Wolff commented that President Clinton had taken the decision away from the USEPA and should be making an announcement at any time. Mr. Harrison affirmed that the USEPA is still under a court order for its rules by the July 19, 1997, deadline.

Dr. Fischer asked what would be a reasonable deadline for finishing the Panel's report. Mr. Harrison indicated that the introductory pages and *Table of Contents* have not been finished but the majority of the report, other than the small section on ozone, has been completed. Changes to current sections and any new material would need to be reviewed by the Panel. Also, it is still necessary to obtain an agreement on the *Major Findings and Conclusions*. Assuming all the above moved along without any problems, two weeks would not be unreasonable.

Dr. Rosenman expressed concern with what he saw as inaccuracies in various sections

of the report as well as with the issue of using non-peer reviewed literature. The review of the National Resources Defense Council (NRDC) study was written by Brown who was hired to critique the NRDC report. Dr. Fischer acknowledged that peer reviewed material was preferable when available. Dr. Kummler argued that the USEPA Clean Air Scientific Advisory Committee (CASAC) report was based on longer study and had more extensive arguments than a certain peer-reviewed article. Dr. Demers stated that to have scientific merit, a minimum standard for the Panel's report must be maintained. At the minimum, non-peer reviewed literature should be cited in the text as being such. Dr. Wolff added that this is a very active area of current research and there is valuable information surfacing which should not be disregarded just for lack of review. Mr. Harrison indicated that the Panel is expected to also rely on its individual and collective expertise to review such material when needed. It can also request the assistance from more appropriately trained colleagues if needed.

Dr. Rosenman identified a book, by Wilson and Spengler (1996), which he felt would be helpful to the report and to the Panel members. Mr. Harrison requested the citation. Mr. Harrison pointed out, given the previous comments, that material from the book, if used, would also need to be referenced as not being peer-reviewed.

Dr. Demers stated that a key decision point of the report was the *Major Findings and Conclusions* as that would set the tone for the whole report. He thought that the current conflict between the *Epidemiology* and *Human Exposure* sections would cause the report not to be taken seriously. If agreement could not be reached between these sections, a minority report would have to be written. He said that the Vedal paper was a good research review. However, portions of it could be taken out of context to make a case either for or against causality. Dr. Fischer mentioned that it is fair to point out the scientific controversy both around the country and within the group..

Dr. Kummler identified what he would like to see as the major findings. First, there are significant human health effects, identified by epidemiological studies, which are associated with some kind of environmental variation. Second is the USEPA's claim that there are statistically significant associations of ambient particulate matter (PM) levels with a variety of human health endpoints. However, the association is weak and there is controversy over confounding factors, biological plausibility and exposure measures. This does not allow a conclusion that PM causes the observed health effects. Dr. Harkema agreed that it is premature to set regulatory changes for PM. He referred to a report, entitled: *In Overview of the EPA's Proposed Revision of the Particulate Matter Standard,* as saying that also. There is not enough toxicological information, and there is a lack of studies with PM_{10} and $PM_{2.5}$. Dr. Wolff stated that after much thought (and questioning by Congress) on this subject as well as consideration of the recent re-analysis studies, he could not accept causality.

Dr. Demers suggested that comments on Dr. Kummler's ideas might be a way to get to common ground. He stated (and Dr. Rosenman concurred) that low odds ratios become significant with consistency. It is not necessary to have all the Bates criteria met to make a case for causality. Causality is never 100 percent. Dr. Demers felt that

too much emphasis was being placed on secondary analyses that do not get the same credit as an original research. And one of the best reanalyses was done by the Health Effects Institute (HEI) which concurred with the major study conclusions. Also, claims for meteorological confounders have not been backed up with any original research or evidence.

Dr. Kummler agreed that more scientific research was needed here as well as about the issues of correlation of indoor/outdoor measurements, $PM_{10}/PM_{2.5}$ agreement, and central/individual monitoring. Misclassification could either hide a real effect or show an association that was not there. He indicated that it has been fairly well established that there are many significant associations with PM.

Dr. Wolff noted that Vedal did not include the reanalysis studies which tend to weaken the consistency. Dr. Rosenman questioned the actual number of reanalysis studies, since the HEI reanalyzed the same data from Philadelphia and got the same results. Dr. Wolff stated that Roth and others re-looked at Philadelphia and other data and that the HEI statement was not consistent with either their results or the editorial by Samet. It was questioned whether separate reanalyses should be counted as different studies.

Dr. Demers explained that human population studies had a larger data set and greater variability than controlled molecular or animal studies. It is fairly simple to take raw data from an epidemiology study and change the findings. Even in clinical trials, there is inherently more weight put on the original hypothesis-driven findings than on a reanalysis. Dr. Kummler countered that the original investigators themselves always must make determinations on which confounders to use or exclude, and an error in judgment here would lead to flawed results. A later study can address more appropriate confounders giving more accurate results.

Dr. Fischer asked whether the hypotheses were the same in the original and the reanalysis studies; that being that PM was associated with increased morbidity and mortality. The re-analyses just used different confounders or different methods of analysis to see if the same association with PM and effect remained. Dr. Demers countered that statistical modeling could change the slant of epidemiology research in either direction. Dr. Rosenman added that many of the reanalyses were still showing associations between air pollution and mortality.

Dr. Wolff questioned whether reducing PM would be sure to reduce mortality. Dr. Rosenman answered that there were a number of uncertainties as to whether the causative factor was actually $PM_{2.5}$. He stated that Lipfert had found $PM_{2.5}$ to be the most accurately measured. However, Dr. Wolff pointed out that Lipfert's position had evolved over the years. In testimony to Congress, he had reported that associations could be shown even if data from different cities were mixed. It was then questioned whether testimony before Congress could be considered scientifically valid. Dr. Demers asked for clarification on what Lipfert had added to his studies in the way of exercise data. Dr. Wolff replied that Lipfert had taken statewide figures and added them to the data set from one of the cross-sectional studies that he had done,

producing a new slope in the results.

Dr. Fischer directed the discussion back to Dr. Kummler's three main points in order to identify as many areas of agreement as possible. Dr. Kummler restated that there are health-related end points that vary from place to place and from time to time. Dr. Demers added that there are health effects but with some unknown source. Dr. Fischer indicated that there were significant health effects that can be statistically associated with PM, but that the cause and effect relationship was not clear. Dr. Kummler stated that his second point addressed that directly and was taken partially from the first of the USEPA findings. The third point was that the association is weak. Considerable controversy over confounding factors, lack of biological plausibility, and the absence of a valid direct exposure measure precludes a conclusion that PM causes the observed health effects.

Dr. Harkema indicated that by saying "lack of biological plausibility" implied testing which provided negative results when in fact the testing had not been done. Dr. Rosenman added that plausibility means possibility rather than proof. Whether it makes biological sense is a different issue than whether there are adequate toxicological data to show a cause and effect relationship. Dr. Wolff suggested using the wording "lack of a known biological mechanism." Dr. Demers agreed. Dr. Rosenman stated that he felt there was not an absence of biological plausibility. He pointed out that the book he had mentioned contained a chapter where they had worked out the effects of PM on lung function. Their results are consistent with the mortality levels that have been associated with PM.

Dr. Rosenman suggested taking all the adjectives out of Dr. Kummler's statement, changing it to: "There is controversy over confounding factors, biological plausibility, and validity of exposure measures." Dr. Kummler, Mr. Harrison, and Dr. Harkema all agreed to that. Dr. Kummler then suggested continuing on with "and hence, controversy over whether PM causes the observed health effects" to address the causality issue. Dr. Rosenman concurred. Dr. Demers stated adding the word "currently" would indicate the dynamic nature of the issue.

Dr. Kummler said that there was a need to add something about the strength of the association, whether it was weak or strong. Dr. Rosenman stated that the odds ratio was weak, but among 250 million people, that was significant. Dr. Demers added the example of mammography, which had statistics of preventing 30 percent of breast cancer deaths for an odds ratio of 1.3. That would be considered a weak association but has been deemed worthy of investing a lot a time and money. Because it is a screening intervention rather than a biological or chemical intervention, biological plausibility is not relevant in this case.

Dr. Fischer questioned the certainty of the number of deaths associated with certain odds ratios. Dr. Demers replied that the certainty was based on solid study design and consistency between studies. There are about 50 studies that show a range of mortality or morbidity excess in the 30 percent to 40 percent range. Dr. Wolff

interjected that the number was actually less than ten, but Dr. Demers maintained that while there were less than ten cohort studies, other types of studies should be included as well. Dr. Rosenman added that there was a good summary table from the book he had mentioned.

When Dr. Fischer asked whether the discussion should be directed at determining the strength of association. Dr. Kummler replied that the question was whether there was controversy over the strength of association with PM. Dr. Harkema added that there was not really controversy about whether there were possible confounding factors. There is justifiable controversy concerning the validity of the statistical association because of confounding factors, proven biological mechanisms of health effects, etc. Dr. Rosenman and Dr. Fischer stated that this would conflict with the first statement of Dr. Kummler. Dr. Rosenman then restated his proposed wording: "There is currently controversy over confounding factors, biological plausibility and validity of exposure measures, and accordingly, controversy over whether $PM_{2.5}$ causes the observed health effects." This statement was agreed to by Dr. Kummler, Dr. Harkema, Dr. Fischer, and Mr. Harrison. Dr. Wolff stated that "observed" should be changed to "recorded."

Dr. Harkema asked again whether there was a statement being made at all about whether the association was weak or strong, to which Dr. Rosenman replied that he was trying to show that a weak association spread among many people was significant. Dr. Kummler stated that even if many more studies showed the same association, there could be a different cause that was simply well correlated with PM. PM can be a great surrogate for many societal ailments that influence public health, including possibly racial injustice, economics, and lifestyle. Accepting the statistical significance of the association, it is still necessary to continue to examine the underlying science to find the true cause(s).

Dr. Fischer proposed that $PM_{2.5}$ exposure would be an expensive program to control given the limited data on health effects and the statistical aberrations. Dr. Kummler added that it was premature. Dr. Rosenman stated that given the evidence available combined with the public concern to "do something," it might be reasonable to do that. Dr. Harkema agreed, saying that current regulations were based on studies that showed the effects of particulates. In this case it was necessary to look specifically at PM_{10} and $PM_{2.5}$. However, Dr. Kummler argued that the data sets on $PM_{2.5}$ were too limited for setting standards. Dr. Fischer asked whether regulation based on PM_{10} had proven to be a reliable surrogate measure and Dr. Kummler replied that there were no studies which showed that the reduction in PM_{10} has resulted in any improvement in health. Dr. Demers brought up the plant closure study in Utah, but Dr. Kummler countered that the study in the next county did not show similar results. Mr. Harrison reminded the Panel that concerns regarding the regulations were not within the scope of the Panel's charge.

Dr. Demers commented that the epidemiological studies have consistently shown statistically significant associations. Dr. Kummler stated that there was still controversy over that. Mr. Harrison brought up the Vedal review and the inconsistencies noted in

the European studies. Both Drs. Wolff and Kummler agreed. Dr. Rosenman disagreed, however, and Dr. Demers stated that there was more consistency than in other topics which the Panel had examined. Dr. Kummler maintained that even if there was consistency, there could be just a good correlation between PM and the real cause. Dr. Demers responded that any set of epidemiological criteria for causality included consistency. Dr. Wolff stated that these criteria were not met.

Dr. Fischer asked for agreement on the meaning of the term "consistency." "Most" was suggested as an adjective to "studies" but Dr. Demers maintained that it was a "very consistent group of studies." "Most" implies only more than 50 percent and there is a vast majority of studies that agree, perhaps 85 percent to 90 percent. Dr. Rosenman suggested using the wording: "Epidemiological studies have repeatedly shown statistically significant associations." Using "repeatedly" was acceptable to everyone. Wording was discussed on the cause and effect issue. Dr. Rosenman suggested saying, "that further research was needed to better clarify the causal relationship."

The NRDC report was discussed with Dr. Rosenman saying that he had no problem with the report. Dr. Kummler brought up the mathematical errors in the report that were pointed out by Jones. Dr. Wolff mentioned that the mean-median error had caused the USEPA to adjust its mortality estimates 25 percent lower. The second error has yet to be addressed by the USEPA. Dr. Rosenman stated that the NRDC methodology was reasonable if you accept its assumptions. Dr. Fischer asked whether the NRDC assumed that the causative agent was PM. Both Mr. Harrison and Dr. Kummler affirmed that it did. Mr. Harrison questioned the validity of the NRDC relating 1989 mortality data to 1990 – 1994 pollution exposure. Dr. Rosenman replied that, accepting the premise that there is an environmental variable associated with health effects, then what the NRDC did is reasonable. Mr. Harrison disagreed by stating that even if the premise is accepted, it is rather difficult to attribute a person's death to exposure received after he is dead. At a minimum, it would require a couple of additional assumptions, which were not addressed in the report.

Dr. Wolff indicated that while there have been some historical environmental disasters that have caused effects, it was not possible to say definitively that there are increased deaths in this country due to current levels of PM. He stated that he was unwilling to accept a causal relationship. Dr. Fischer replied that there was something that was impacting human health and that it could be impacting a large group of people. Dr. Kummler summarized that assuming there was a cause and effect relationship, and including caveats about errors and the timeliness of the data on exposure, then it is acceptable to say that a large number of people would be impacted. However, what is seen is repeated consistency, but not causality.

Dr. Fischer expressed concern that there could be conflict between broad statements being generated by the Panel and the definite statements being written by individual members. Dr. Demers restated his concern with the conflict between the *Epidemiology* and *Human Exposure* sections.

Dr. Fischer suggested that it would be useful for Dr. Wolff to clarify his section to identify the fact that it was a review of what had been done for CASAC. Dr. Rosenman pointed out that the report section was different than the summary letter for CASAC. Dr. Wolff replied that other things had come out since that letter had been written. Mr. Harrison asked for clarification on the usability of non-peer reviewed material by Brown, Jones, and the book that Dr. Rosenman had suggested. Dr. Kummler stated that Jones's comments were important to include because they had been accepted by both Pope and the USEPA and they included notice of numerical errors in some of the original data. Also, using the magnitude of today's air quality to judge historical air pollution and effects would contribute additional error. Dr. Fischer indicated that it would be useful to indicate the non-peer reviewed status of this literature in the report.

Mr. Harrison indicated that Dr. Wolff has been asked to prepare the ozone portion of the report for everyone to review. He indicated that once he received the draft, it would be sent out to the other Panel members for review.

Dr. Fischer suggested that changes to the report and new sections of the report would be sent to the Panel members for comment and concurrence. It was also suggested that those members with outstanding disagreements should contact each other and resolve these differences. Once an agreement is reached, the final wording could then be sent to Mr. Harrison.

0 PUBLIC COMMENT

There was no public comment.

IV. NEXT MEETING DATE

No additional meetings were scheduled.

V. ADJOURNMENT

The meeting was adjourned at 12:45 PM.

Respectfully submitted,
Keith G. Harrison, M.A., R.S., Cert. Ecol.
Executive Director
Michigan Environmental Science Board